Retention Myths vs. Well-Managed Resources: Promises and Failings of Structural Realism

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Abstract. Turning away from entities and focusing instead exclusively on 'structural' aspects of scientific theories has been advocated as a cogent response to objections levelled at realist conceptions of the aim and success of science. Physical theories whose (predictive) past successes are genuine would, in particular, share with their successors structural traits that would ultimately latch on to 'structural' features of the natural world. Motives for subscribing to Structural Realism are reviewed and discussed. It is argued that structural retention claims lose their force if one gives up merely historical readings of the transition from Galilean-relativistic classical mechanics to the 'special' theory of relativity, heeding instead basic requirements that lead to their common derivation. Further cause for scepticism is found upon realising that the basic mathematical framework of quantum theory essentially reflects its predictive purpose, without any necessary input, be it of a 'structural' kind, from the physical world.

1. How physics survives pessimistic induction

Not only is realism towards events and things a 'default' attitude; it is also widely believed to be given support by the overwhelming success of science and the ubiquity of its technological by-products. How could we know and be able to *do* so much if our scientific theories did not somehow 'reach out' to the being and properties of a pre-existing objective world, and inform us reliably and truthfully about them? Expressed in a respectable academic format, this rather commonplace and straightforward doctrine goes by the name of *Scientific Realism*. Its proponents insist that, insofar as our scientific theories provide a successful account of phenomena, what they tell us about the entities they invoke – electric fields, photons, genes and so forth – must capture something of what those entities truly are, their properties and their (inter)actions. A worthy, 'mature' scientific theory provides, if not a true picture of reality, at least an approximately true one. The entities it posits, or quite similar ones, do exist in the world, and what the theory tells us about their attributes and behaviour is a broadly correct approximation to the truth, albeit one that may always be improved on[1].

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[1] Scientific realists will readily concede that not everything in a theory may approximate truth to the same degree. A mathematised theory, for example, may include formal 'gears and wheels' that,

Empirical success is what sifts out 'good' theories from more or less worthy competitors, and the more enduring their successes, the more trusted are the winners. In the eyes of the realist, success stories are tell-tale signs that certain objective features of physical reality are properly, if only approximately accounted for. As the critics have not failed to point out, empirical evidence underdetermines theories, so that it can never be asserted for sure that any one is actually 'closer to truth' than other similarly successful contestants. Nonetheless, as supporting evidence accumulates, it gets ever more unlikely for credible competitors to catch up. Isn't the winning theory simply that which, to date, yields the best fit with reality? Despite massive critical fire, the realist remains convinced that the position (s)he defends is that which makes the best possible sense of indisputable and persistent success. This 'inference to the best explanation' is usually paired with what, in its most radical form, has come to be known as the 'no miracles argument' (NMA): Scientific Realism is, purportedly, the only doctrine that does not make scientific success sound like a miracle[2]. In a slightly attenuated form, it would be the only account of theories that does not make their success exceedingly improbable. This 'Greatest Likelihood Argument' (GLA) succumbs to the base rate fallacy: depending on what probability value (as a measure of one's degree of belief) one wishes to assign *a priori* to a given theory, any posterior, evidence-based probability that the theory in question is 'on the right track' can follow, including one that is as low as one likes– or rather wouldn't like, if one is a convergent realist[3]. Whether or not one cares to quantify it, however, the NMA/GLA is above all the expression of a strongly-felt hunch; one that is so ingrained that the realist is not going to give it up without a fight. In fact, because of their unshakable commitment to the idea of an external world as both the source and ultimate target of scientific knowledge, most realists simply do not believe any serious alternative to their doctrine is even worth considering.

It is tempting to think that our well-corroborated theories simply yield approximate truth about reality. However, didn't past 'scientists' feel just the same about their own cherished, but defunct theories? Ptolemaic astronomy prospered for centuries, in spite of its getting basic facts wrong. Closer to us, Newton's mechanics and gravitation theories, once held to be the pinnacle of human achievement, reached their limits on various grounds and were replaced by theories whose premises and methodologies are fundamentally different. Who knows whether the theories we now favour will not end up, like their predecessors, on the scrap heap?

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in and of themselves, do not refer to anything in the world, but whose 'syntactical' operation may none the less be required for the theory to perform its duties.

[2] H. Putnam, 'What is Mathematical Truth', in *Mathematics, Matter, and Method, Philosophical Papers* Volume 1, Cambridge: Cambridge University Press 1975, pp.60-78.

[3] For a brief discussion, see J. Worrall, 'Miracles and Models: Why reports of the death of Structural Realism may be exaggerated', in A. O'Hear (ed.), *The Royal Institute of Philosophy Supplement:* ***61***, Cambridge: Cambridge University Press 2007, pp.136-140. Worrall acknowledges the objection but chooses not to regard it as threatening his faith in the necessity of (some variety of) realism.

Some antirealists have argued that the historical record gives us serious cause for pessimism regarding the ability of our current theories to approximate truth. Larry Laudan's contribution to the debate includes a list[4] of past 'scientific achievements' we now regard as hopelessly off beam. Laudan's list aims to render the NMA impotent by displaying a significant number of counterexamples. If the inherent fallibility of inductive arguments prevents it from being the last word, the ensuing inference to the irrationality of convergence claims, sententiously known as *pessimistic (meta-)induction,* would still be a severe setback for Scientific Realism.

Laudan's list is intended to impress. On closer inspection, however, does it really hold its promise? About a quarter of the list pertains to what we might charitably call 'paleobiology': the humoral 'theory' of medicine, 'theories' of spontaneous generation, the vital force 'theories' of physiology. If those do qualify as *conceptions*, it is debatable whether they qualify as theories, let alone scientific ones, given our current understanding of what scientific theorising involves. They are merely plausible and to a large extent *a priori* stories about living organisms or the human body. So are the claims of catastrophist 'geology' about the Earth and its changes of appearance over millenia. None of those 'theories' relies on cautious experimentation, nor do they make use of the kind of highly controlled and constrained reasoning we have come to associate with the scientific method. What is more, paying tribute to such influential, venerable figures as Galen or Aristotle – not to say anything about allegiance to scriptures – is rife and stands in sharp contrast to the 'find out for yourself' attitude that has (ideally!) been that of scientists since, say, Galileo. As for their success, well: who did survive treatment in Molière's days would most likely have recovered better and earlier without.

Moving down the list, crystalline spheres are nothing but a concession to Aristotle's metaphysics, which abhors the idea of a perfect vacuum – hence the heavenly fill-up. Ptolemaic astronomy itself is a mixture of traditional cosmological conceptions and ingenious, *ad hoc* algorithmic procedures. Despite its computational merits, it is hard to confer scientific status to it, just as we should refrain from regarding Mayan 'astronomy' as a science, regardless of the predictive accuracy of some of its 'methods'. If Kepler's carefully worked out statement of three basic laws of planetary motion qualifies as scientific, can his earlier Platonic speculations (*Mysterium Cosmographicum*) count as an example of theory? Are phlogiston theory, the caloric theory of heat and the effluvial theory of static electricity more worthy candidates? The phlogiston idea originates in Johann Blecher's 1667 *Physica Subterranea*, in which he postulates the existence of a 'natural element' – so-called *terra pinguis* – he imagines to be released in the process of combustion. Its *ad hoc* character makes phlogiston an unlikely candidate as the central

[4] L. Laudan, 'A Confutation of Convergent Realism', *Philosophy of Science* **48**, 19-48 (1981); reprinted in D. Papineau (ed.), *The Philosophy of Science*, Oxford: Oxford University Press 1996, pp.107-138.

concept of a bona fide scientific theory[5]. The predictive success of the phlogiston view of combustion is not easy to ascertain, let alone quantify, because the theory lacked the support of substantial mathematisation. It probably owes its longevity and limited 'success' to a coincidence: phlogiston was imagined to operate as a sort of negative of oxygen; what chemistry would understand as consumption of oxygen was regarded as phlogiston release. As for Gilbert's effluvial conception of static electricity, a look at his *De Magnete* (1600) betrays its Aristotelian persuasion. This is, again, a work of imagination that can at best support a 'narrative' whose plausibility must be evaluated in its historical context.

Unlike phlogiston or effluvia, the caloric idea did contribute to undeniable scientific developments. It did enjoy a fair amount of explanatory success (e.g. it does rather neatly account for the cooling of a hot liquid at room temperature) and could provide some justification for the gas laws. This and the lack of any clear and convincing alternative picture of heat led such prominent scientists as Carnot and Laplace to endorse it. Nevertheless, Carnot's accomplishments were (thankfully) independent of what he thought heat *was*. Laplace's rather loose commitment to the caloric view was certainly useful in suggesting to him the addition of a constant (currently known as the 'adiabatic index') to the equation Newton had derived for the propagation of an acoustic disturbance in an elastic medium[6]. This resulted in a substantial correction to Newton's prediction of the speed of sound[7]. Thus caloric, as a guiding idea, was a useful fiction: it provided an incentive for refining the mathematical models of acoustic phenomena, and a fairly neutral background for reflecting, as Carnot did, on the 'motive power of fire.'

We no longer believe in caloric, in phlogiston or effluvia. Neither are we greatly impressed by what the theories that invoked them achieved. If they were successful at all, whatever successes those theories did score range from plausible explanations (as judged, at least, by the standards of their times) to satisfactory quantitative predictions. But the latter were the products of great ingenuity rather than reflections of their authors' beliefs. Laudan makes much of the empirical success of Fresnel's optics. This theory accurately predicts interference or diffraction effects, some of which run counter to uninformed expectations. The realist might seize the opportunity and highlight this as evidence that genuine aspects of the physical world have been captured by Fresnel's account; aspects

[5] "This theorizing as to phlogiston resembles in its methods the dreaming of the Greek philosophers, who preferred to base their theories on pure reasoning rather than on observation and experiment. No attempt at first was made at the isolation of phlogiston, nor were experiments adduced in support of the theory." (F. Preston Venable, *A Short History of Chemistry*, Boston: D.C. Heath & Co. 1894, p.52).

[6] Finn B.S. 1964. 'Laplace and the Speed of Sound', *ISIS*, Vol.55, No.179, pp.7-19.

[7] Newton had assumed that pressure changes in an acoustic disturbance are simply proportional to density changes (*Principia*, Book II, Proposition 49), which was both plausible and computationally convenient. Proportionality, however, would hold only if temperature remained constant throughout, but it is not. Regarding heat as a substance rather than as a manifestation of molecular motion does not affect those considerations: pressure and temperature are quantities that are, in either view, given a clear operational meaning.

that appealing to common sense or ordinary observation (as opposed to careful experimentation) would not be capable of revealing. There's the rub, however, for despite the success of Fresnel's predictions, the ontology he advocated (wave-like disturbances propagating in an all-pervading elastic medium) is now believed, with good reason, to be utterly mistaken. Ontologies are therefore not, Laudan concludes, necessary or even relevant to predictive success. He does, however, misrepresent Fresnel's achievement, making it sound as if Fresnel's belief in optical ether was actually instrumental in his account of diffraction. Given the fate of that particular ontology, wouldn't such dependence make his striking and enduring success very implausible? What made Fresnel succeed was not speculation about the nature and properties of a medium for the propagation of light; rather, it was the effective development of key ideas, such as periodic variation, and how he drew fundamental implications of Huygens's principle. It is as a seasoned practitioner of the art of model-making, and not as a speculative thinker, that Fresnel succeeded in 'squeezing out' correct predictions. This is how physicists and engineers work at their best, and certainly the reason why some of the relations Fresnel derived have outlived his ontological preconceptions.

All in all, one must admit that, if 'striking predictive success[es]...is a precondition of acceptance[8]', then most items on Laudan's list simply do not qualify. Most significantly, those few theories that were undeniably successful in predicting phenomena, both quantitatively and qualitatively, happen to fit in, however loosely, with our current conception of *physical* theories. Those are the theories that did not die in vain, i.e. the theories whose successful predictions have been least affected by the demise of putative ontologies: Fresnel's optics, Newton's mechanics, Maxwell's electrodynamics... This resilience has very much to do with their hinging on the expert and judicious exploitation of mathematical resources. What remains to be seen, however, is whether focusing on that particular aspect can help rescue Scientific Realism or lend support to some variant thereof. Should one wish to follow the lead and come up with a 'structural' (presumably weakened) variety of realism, it must be borne in mind that the scope of the endeavour will be confined to *physical* theories – at least until it is proven to be legitimate to extend its theses to other recognized scientific domains.

[8] J. Worrall (2007), *op. cit.*, p.127.

2. Realism goes structural

Those, like Laudan, who dwell on 'ontological losses' suffered as a result of radical theory change tend to exaggerate – if only for denying it – the importance of metaphysical baggage as a factor crucial to the effectiveness of *physical* theories. Besides, there does at least *appear* to be a substantial carry-over, at some level, from one worthy physical theory to the next. If such inheritance, or retention, is not illusory, it would occur despite the loss of 'central terms that (we now believe) were nonreferring[9].' Its occuring at a much more abstract, 'structural' level, would make such retention relatively insensitive to the fate of ontologies. In his essay *La science et l'hypothèse*, Henri Poincaré downplays the importance of postulated entities, such as Fresnel's ether or Lorentz's electron. He regards them as convenient fictions and contrasts them with the enduring truth of laws and equations:

> 'Never mind whether the ether really exists, for this is the business of metaphysicians; what matters for us [physicists] is that everything happens as if it did exist and that this hypothesis is convenient for the explanation of phenomena. After all, do we have any other reason to believe in the existence of material objects? There again, it is merely a convenient hypothesis; however, it will never cease to be, whereas one day will no doubt come when the ether is rejected as useless[10]. But on that very day, the laws of optics and the equations that express them analytically will remain true, at least as a first approximation. It will therefore be useful to study a doctrine that connects all those equations[11].'

Despite the eventual rejection of Fresnel's and other theories, 'the differential equations [the authors of those theories came up with and successfully used for making predictions] are always true, they may always be integrated by the same methods and the results of this integration still preserve their value[12].' Hypotheses that led to deriving those equations may well have been unwarranted beliefs or illusions, but they somehow served as a guide to their derivation. Be it for that reason only, the doctrines in question were certainly not held in vain.

Following Poincaré's insights, John Worrall claims that what distinguishes our most successful scientific theories is their being 'structurally correct[13]'. Moreover, 'this is the strongest epistemic claim about them that it is reasonable to make[14].' This provides a motive and grounds for a variant of Scientific Realism, which has come to be known as

[9] Laudan, *op. cit.*, p.121.

[10] Poincaré writes those lines in 1889.

[11] H. Poincaré, *La science et l'hypothèse,* Paris: Flammarion 1968, p.215 (my translation).

[12] H. Poincaré, *Science and Hypothesis* (*SH*), New York: Dover 1905, note 9, p.160.

[13] J. Worral (2007), *op.cit.* p.125.

[14] *Ibid.*

Structural Realism[15]. Notice again, though (this is quite clear from Poincaré's statement and preoccupations) that the case for Structural Realism (SR) seems to be restricted to physical theories, or to those scientific theories that would share with physics a very definite, 'special' kind of relationship to mathematical concepts and methods (see Section 3). What does it mean, then, for a theory to be 'structurally correct'? In Poincaré's own words: 'if the equations remain true, it is because the relations preserve their reality[16]'. Not only must a compelling, 'simple' law transcend the somewhat gratuitous nature and unreliability of hypothesis choice, but the relations Poincaré alludes to would 'encode' and preserve, transtheoretically, reference to a certain 'something out there' that would transcend theory change[17].

According to Worrall and his fellow 'structuralists', putative qualities of physical objects would be mostly irrelevant to the operation of a physical theory, and therefore to its predictive success. Referring to such qualities would yield at best a metaphysical picture, an ontological gloss whose main virtue – definitely a mixed blessing – would lie in its stimulative power and in the incitement it may provide the theorist to develop ideas and 'try them out'. With respect to the hypothetical furniture of the world, one could not do any better than to remain agnostic. One's trust in a theory should not be placed in anything but in its mathematical, 'operative' content; for the latter only has, and can have, a significant and provable impact on predictive success. This position strongly contrasts with Scientific Realism in that the cognitive, epistemic import of physical theories is restricted to their mathematical makeup and what follows from it in terms of empirical (predictive) consequences. What makes it a variety of realism is the structuralist's belief that physical theories are successful, and they can *only* be, *because* their key operative structures somehow 'reflect' or latch on to correspondingly 'structural' aspects of the physical world.

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[15] J. Worrall, 'Structural Realism: The Best of Both Worlds?', *Dialectica* **43**/1-2, (1989); reprinted in D. Papineau (ed.), *The Philosophy of Science*, Oxford: Oxford University Press 1996, pp.139-165.

[16] *SH*, note 9, p.161.

[17] Although he mentions in this context 'true relations between real objects' (*SH*, note 9, p.161), it is debatable whether Poincaré would have wholeheartedly embraced SR. The nearest he might have come to be a realist might well have been in his sharing a common hunch: that the network of relations (between definite quantities) which physicists work out and validate via careful experimentation tends towards the ideal of a 'natural classification' (P. Duhem, *The Aim and Structure of Physical Theory*, Princeton: Princeton University Press, 1991; translated from *La théorie physique ; son objet, sa structure* (1906)); something as objective, hence 'natural' as our endeavours can possibly get to.

In order to assess the case for Structural Realism, it will be convenient to break it down into three theses:

(i) The primary, unshakable credo of the realist: a metaphysical claim about the existence of a pre-organised reality. In the words of Elie Zahar[18]:

> 'There exists a *structured* reality of which the mind is a part; and, far from imposing their own order on things, our mental operations are simply *governed* by the fixed laws which *describe* the workings of nature.'

(ii) *Transtheoretical Retention* (TTR):

Regardless of the differences in their basic hypotheses, successive [physical] theories that register significant success in predicting phenomena have in common certain *structural* traits e.g. in the form of similar or even identical equations. The similarity and retention of those traits cannot be sensibly ascribed to chance.

(iii) *Inference to the best, 'structural' explanation* (IBSE):

TTR makes it reasonable to believe that such theories as considered in (ii) afford access to genuine aspects of the reality posited in (i). Access is provided *only* through those structural traits of theories that withstand radical changes in their ontological premises, and it consists in some sort of correspondence between those traits and suitably 'structural' features of physical reality.

Whilst (i), or a similar claim[19], is common to all brands of realism, (ii) and (iii) are distinctive of Structural Realism. TTR – the persistence of certain structural elements in the face of theory change, however radical that change might be at a hypothesis level – would be a tell-tale sign that the elements in question capture something genuine, of a perhaps essentially 'relational' nature, about the world. No less, and certainly no more. Much of the strength of structural realism would lie in its making no stronger claims than can withstand Laudan-style arguments. As J. Worrall puts it, 'If SR isn't realism then nothing defensible is[20]'. If this is so, then if SR is found ultimately wanting it is the very fate of realism as a justification for the success of (physical) science that must be called into question.

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[18] E. Zahar, *Poincaré's Philosophy: From Conventionalism to Phenomenology,* Chicago: Open Court 2001, note 21 p.86. Emphasis added.

[19] Zahar's statement, with its reference to 'laws' that both 'describe' and 'govern' the workings of nature *and* our cognitive processes, is open to debate, and certain realists might wish to substitute a more cautious, albeit fundamentally equivalent formulation of their core belief.

[20] J. Worral (2007), *op.cit.*, p.154.

TTR, and therefore Structural Realism itself, is particularly, if not exclusively, a claim about *physical* theories. Given that 'structural' is, more often than not, used by structuralists as if it were quasi-synonymous to *mathematical*, a review of the role of mathematics in the formulation – indeed, in the very constitution – of physical theories is in order before theses (ii) and (iii) can be addressed and criticised. Such a review is the object of the next section.

3. Physics is 'mathematical'

However important its role, e.g., in biomedical research, mathematics merely lends this and other scientific domains the precision of its techniques and a capacity to yield quantitative predictions. This instrumental role very strongly contrasts with the thorough mathematisation of physical concepts. This is so down to the very basics: without the notion of derivative, there is no effective idea of an instant velocity. Introducing and coordinating physical quantities also relies on specific structures (in the precise, mathematical sense of the word): for example, it is part and parcel of the classical concept of velocity for it to be conceived and manipulated as a *vector* quantity. How the word *energy*, used e.g. in casual conversation, has come to mean everything and nothing shows, *a contrario*, how a legitimate physical concept becomes vacuous when it is stripped of its precise mathematical expressions.

It has often been pointed out – more to wonder about the fact than as a starting point for trying to elucidate an intriguing aspect of the mathematics-physics relationship – how twentieth-century physics has capitalised on the availability of concepts and structures that mathematicians had developed without any application in mind but for the sake of aesthetic appeal or the intellectual challenge of working out consequences of their axioms. Tensor calculus and Riemann's differential geometry happened to provide just the right tools and concepts Einstein needed to be able to work his way from the postulation of an 'equivalence principle' to a fully local and relativistic theory of gravitation. Born's recognition of matrices in Heisenberg's 'tables' or 'assemblies' (*Gesamtheit*) is another striking instance of mathematical conquests or inventions being available to meet the needs of the physicist. Conversely, developments in theoretical physics have occasionally stimulated the production of new mathematics or suggested fruitful avenues for mathematical research. Thus, while Fourier's treatment of heat belongs to history, the powerful techniques he developed to solve a particular partial differential equation have become essential items in an ever-expanding mathematical toolbox. In view of some scathing criticism[21], it might also well be that work on currently fashionable superstring theories will mostly benefit the mathematician.

[21] e.g. that of R. Penrose, *The Road to Reality*, New York: Vintage Books, 2007.

A notorious cause for wonder is the fact that similar expressions, e.g. differential equations, occur as the outcome of addressing what may look like radically different problems. Feynman, for one, calls it 'a most remarkable coincidence[22]'. Those similarities come in handy, for 'having studied one subject, we immediately have a great deal of direct and precise knowledge about the solutions of the equations of another[23]'. For example, knowing the equations of electrostatics 'you have learned at the same time how to handle many subjects in physics, and…keeping this in mind, it is possible to learn almost all of physics in a limited number of years[24]'. This is indeed extremely convenient, but the question remains: '*Why are the equations from different phenomena so similar?...* The electrostatic potential, the diffusion of neutrons, heat flow – are we really dealing with the same stuff?[25]' If he makes a passing and evasive reference to 'the underlying unity of nature[26], Feynman knows his subject too well to ignore that the form of differential equations is determined, not by the kind of 'stuff' it refers to, but by the assumptions the physicist decides, or feels obliged to make:

> Is it possible that…the thing which is common to all the phenomena is… the framework into which the physics is put? As long as things are reasonably smooth in space, then the *important* things that will be involved will be the rates of change of quantities with position in space. That is why *we always get* an equation with a gradient. The derivatives *must appear* in the form of a gradient or a divergence; because the laws of physics are independent of direction, they *must be expressible* in vector form. The equations of electrostatics are the simplest vector equations that one can get which involve only the spatial derivatives of quantities. Any other *simple* problem – or *simplification* of a complicated problem – must look like electrostatics. What is common to all our problems is that they involve space and that we have imitated what is actually a complicated phenomenon by a simple differential equation[27].

If Feynman speaks about 'imitation', it is because his realist leanings prompt him to regard the outcome of model-making as 'a smoothed-out approximation to a mechanism underneath[28]'. Nevertheless, the occurrence, in the treatment of seemingly unrelated problems, of equations whose form turns out to be the same may have little, if anything, to do with the nature of whatever unknown mechanisms those equations may or may not 'approximate'. The basic assumptions the theorist ends up relying on: slow variation, neglect of higher-order terms, isotropy, homogeneity etc. are expressions of general,

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[22]R.P. Feynman, R. Leighton and M. Sands: *The Feynman Lectures on Physics*, Vol.2: *Electromagnetism*, Reading MA: Addison-Wesley 1965. 12-1.

[23] Feynman *et al.*, *op. cit.*, 12-1.

[24] Feynman *et al.*, *op.cit.*, 12-12.

[25] Feynman *et al.*, *op. cit.*, 12-12. Italics are Feynman's.

[26] *Ibid.*

[27] *Ibid.* Italics added.

[28] Feynman *et al.*, *op. cit.*, 12-13. However, he acknowledges difficulties with such a view in e.g. quantum electrodynamics.

perceptually or cognitively motivated expectations, and operational (e.g. computational) necessities as much, at least, as they are responses to the 'pressure' of an external world. Such assumptions actually are a *conditio sine qua non* for mathematical formulation and effective treatment. Indeed, it takes very slight 'corrections' to make a set of equations impossible to solve. Whatever appearance of unity the similarity of equations points to should not, therefore, hastily be ascribed to nature. Such unity might rather be thought of as characterising the range of our possibilities of conceiving and effectively handling those distillates of (possible or actual) experience the physicist's models are and can only be. Striking as they are, all observed similarities boil down to some basic requirements for intelligibility, together with the concessions that must be made for an optimal use of the conceptual means and powerful methods mathematics affords.

Productive work in theoretical physics is very dependent on some carefully thought-out choices of representation. Second only to those choices is skillful use of well-designed and flexible symbolic notations. With its unique capacity to harness thought and carry it along in the safest possible vehicle, a consistent and well-designed formalism allows its user to develop all the consequences of an initial set of assumptions. Well-tried computational procedures all but dispense one to reflect, at every step, on what the manipulated symbols actually refer to, in terms of their relationship to entities or attributes. Once properly set on its course, under the vigilant eye of the expert, mathematisation leaves no freedom to improvise. Barring computational errors, one cannot be led astray without having betrayed one's starting assumptions. Such may be the intricacy of what those assumptions imply that the most insightful and best-trained mind can do no more that guess at plausible outcomes. However, difficulties with analytical treatment notwithstanding, the iron hand of the formalism ensures that those consequences can be *worked out*.

If there is a constructive role for ontological preferences to play in the matter it consists in making, through a mental picture or 'narrative', some of the key requirements or assumptions explicit (this is, of course, far from being foolproof). What matters most is that the physicist be led to decisions that will be just those required to yield exploitable equations. Thus, reasoning that is 'channelled' by a flow metaphor may lead – once all the attendant assumptions have been precisely formalised – to similar partial differential equations, whether the object of the model is the motion of an actual fluid, cash flow in financial markets or traffic flow on road networks. Fresnel's mental picture of a 'rippling' medium somehow enabled him to arrive at successful predictions. If later physicists could 'recover' Fresnel's results in the context of their own 'electromagnetic world picture', this is because the latter prompted them to make essentially similar moves, when they were faced with the challenge of turning their insights into a *workable* model. This is how assumption-driven effectiveness can give appearances of a reality-driven transtheoretical continuity.

Bearing in mind the specific character of the physics-mathematics relationship, appearances of TTR are both significant and deceptive: they are significant in pointing to the unity – and, by the same token, to the limited range – of the highly consequential decisions the theorist must make in order for his abstracted account of phenomena to be effectively exploitable. But they are deceptive in that they lend themselves, when considered uncritically, to the simplistic view of a common thread that would run through successive theories, bearing the ‘structural’ imprint of an external world.

Models and theories of specific phenomena have to comply with the requirements of basic theoretical frameworks – *frame theories*, as we shall call them here. The transition from one frame theory to another involves no TTR *stricto sensu*: basic classical equations did not survive the relativistic or quantum tidal waves. Instead, any links between those theories are usually exhibited in some appropriate limit. However, as we shall see at length in the next section, approaching them from a different perspective, free from the contingent lessons of history, leads to a very different view of the mutual relationship of classical mechanics and ‘special relativity’ on the one hand, and the purpose of quantum theory on the other. This will affect both our assessment of the value of correspondence claims and our judgment about the plausibility of the structuralist’s IBSE (thesis (iii) above).]

4. Beyond correspondence: revisiting the foundations of our best frame theories

(i) Relativity without light

Einstein's ‘special’ theory of relativity (STR), as it is used and taught just about everywhere, is based upon two postulates: (i) The mathematical form of the laws of physics must be the same in all inertial reference frames; and (ii) The speed *c* at which light – or more generally electromagnetic radiation – propagates *in vacuo* happens to be the same in all inertial frames. Whilst (i) is a general claim about the indifference of basic theoretical statements (physical laws) with respect to changes of reference frame (as long as those frames are in uniform translational motion relative to one another), (ii) is an assertion about a particular manifestation (light) of a given interaction (electromagnetism).

Postulate (i) actually is entirely compatible with Galileo’s views about relative motion. On the other hand, since (ii) flatly contradicts ‘Galilean’ relativity (e.g. the additive composition of *all* velocities), blocking from the outset a ‘Galilean transformation’ Galileo himself never derived, a theory based upon (i) and (ii) is bound to be inconsistent with Newtonian mechanics, in which Galilean relativity is tacitly assumed. If the latter can no longer be regarded as true, it does remain, of course, very useful for dealing with cases where typical velocities (*v*) have much smaller magnitudes than the speed of light *c*. This is sanctioned by the invocation of a ‘correspondence principle’ whereby the Galilean tranformation is ‘recovered’ in the $\frac{v}{c} \to 0$ limit. The conceptual chasm between the two frameworks remains, however, breeding comments about their radical dissimilarity, if not their mutual ‘incommensurability’.

Despite the magnitude of Einstein's achievement, the feeling might remain that his original and now standard two-postulate formulation is not, because of (ii), entirely faithful to relativistic objectives. This is all the more so since, as any textbook derivation of the Lorentz-Einstein transformation will readily confirm, the form of that transformation owes everything to the second postulate, which requires the speed of light to have the value c in all inertial frames. The relativity postulate (i) is, in itself, too general to help constrain, let alone determine, the form of change-of-frame transformations. It merely serves to motivate the search for a universal transformation. Given the weight of (ii) in its derivation, the Lorentz transformation may not appear to be so much a clear embodiment of relativity as a surprisingly far-reaching consequence of a striking fact about light.

It may then come as a surprise that both the Galileo and the Lorentz-Einstein transformation can be derived from the *same* set of assumptions[29], without any reference to light and its propagation (or indeed to any other kind of physical process). Following Galileo's insight: mechanical, or more generally physical 'laws' should not depend in any essential manner on the state of motion of whoever describes them – as long as one sticks to so-called inertial frames – the aim is to work out the general form of the transformation of space coordinates and time that regulates passage from one inertial frame to another. The sought transformation should guarantee that any physical quantities, as functions of coordinates in any one such frame, are systematically transformed into corresponding functions of the coordinates in another inertial frame.

There are no grounds, empirically or otherwise, for regarding space as inherently polarised (which would make certain directions *a priori* different from others). This is all the more so if space is regarded as an abstract and convenient framework for locating objects. We are then free to choose the direction of relative motion of two inertial frames as that which coincides with an arbitrary, conventional x axis. Specifying the transformation rule that regulates passage from an inertial frame R to another one R' will then essentially amount to working out the precise form of two functions of just one space (x) and one time (t) variable: $x' = F(x,t)$ and $t'=G(x,t)$. It is also reasonable to expect the transformation of any space or time interval not to depend on 'where' that interval is located (all intervals of a given amplitude a should transform by the rule into intervals of the same amplitude a'). This homogeneity requirement strongly constrains the form of F and G: to fit the bill, those must be linear functions of their inputs, i.e. $x' = \gamma x + \delta t$ and $t' = \alpha x + \beta t$. Taking explicity into account the relative motion, with velocity $\pm v$ in the x direction, of the

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[29] Intellectual inertia and Einstein's fame as the foremost scientific hero of our times have been quite successful in keeping the public and even most practitioners of STR unaware of this 'non-secret'. For examples of its periodic rediscovery, see J.-M. Lévy-Leblond, 'One more derivation of the Lorentz transformation', *Am. J. Phys.* **44**, 271-277 (1976), A. Sen, 'How Galileo could have derived the special theory of relativity', *Am. J. Phys.* **62**, 157-162 (1994), and M.J. Feigenbaum, 'The Theory of Relativity – Galileo's Child', ArXiv.org e-print 0806.1234 (2008). On a personal note, the dissatisfactions expressed above resurfaced – leading to the selfsame 'rediscovery' – as I was lecturing (University College London, 2000) on the emergence, in historical context, of Einstein's 'special' theory.

two frames then reduces from four to two the number of unknown coefficients, each of which is a function of v only[30]. The precise form of those functions follows if the transformations are finally required to combine among themselves as the elements of a group: in particular, if R , R' and R'' are any three inertial reference frames in relative rectilinear motion, then any given '*F*,*G* transformation' from R to R' followed by one transformation of the same type from R' to R'' should be equivalent to a single *F*,*G* transformation between R and R''. The form of F and G is thereby completely determined:

$$x' = \frac{x - vt}{\sqrt{1+\lambda v^2}} \qquad\qquad t' = \frac{t + \lambda v x}{\sqrt{1+\lambda v^2}}$$

Three possibilities follow, depending on the sign or the value of the parameter λ. The λ=0 option yields the Galileo transformation, which Newton's mechanics implicitly assumes and which appeared to be borne out by all experiments until the last decades of the nineteenth century. A strictly positive value can be ruled out in view of its anomalous consequences. For example, composing the velocities of two motions in the same direction (v>0 and v'>0) according to the law: $v'' = \frac{v + v'}{1 - \lambda v v'}$ (which follows from the assumptions) could then result in relative motion in the opposite direction (v''<0) !

The dimensionless $\sqrt{1+\lambda v^2}$ factor in the remaining case (λ<0) is a function of the relative velocity v of the two frames. Since λ happens to have the dimensions of the inverse of a velocity κ squared, the appearance in the corresponding 'Lorentz' transformation of the $\gamma(v) = \frac{1}{\sqrt{1-\frac{v^2}{\kappa^2}}}$ factor can be interpreted as implying a 'structurally' imposed limit κ to the relative velocity of two inertial frames. A convenient correspondence between the two λ<0 and λ=0 frameworks is thereby established, in the form of a smooth transition from one framework to the other in the $\frac{v}{\kappa} \to 0$ limit. The procedure comes in handy and it may be 'reassuring', but this changes nothing to the mutual exclusiveness of the λ=0 and λ<0 cases.

Now suppose, for the sake of argument, that the above derivation had been achieved prior to that of a package of equations for electromagnetism. If we assume in this context – rather unrealistically – sufficiently advanced technology (accurate clocks, and perhaps fast flying machines), comparison of the readings of previously synchronised clocks[31] after

[30] Given that the transformation itself must be unaffected if relative motion is time-reversed ($t \to -t$) or a mirror image of it is considered (which amounts here to $x \to -x$), it follows that one of these functions is odd, and the other even.

[31] Despite Einstein's famous discussion of clock synchronisation (in the first part of his 1905 *Zur Elektrodynamik* article), the procedure through which this is achieved need not involve light – and why should it? If Einstein makes sure that it does, this is because he is primarily concerned with the consistency of electromagnetism with mechanics. Galileo had actually come up with an ingenious – and obviously light-free – modus operandi for ensuring that clocks are properly

they had been set in relative motion could have revealed discrepancies between those time readings, thereby calling into question the 'obvious truth' of the Galilean conception. Given the relative velocity v of the two clocks, an empirical evaluation of λ would have immediately followed from the ratio $\sqrt{1+\lambda v^2}$ of the two times, without any incongruous reference to electromagnetism or the propagation of light. If physicists had then cared to express that ratio in terms of κ, two-clock data would have revealed that this theoretical limit is very high compared to speeds that had ever been experienced, or that one might even reasonably expect to experience. Had one, however, measured the speed of light, coincidence of its value *in vacuo* with the empirical evaluation of κ would certainly have come as a major surprise[32]. The subsequent realisation that an adequate set of equations for electromagnetism is covariant under a 'κ-transform' would then have been welcomed as evidence of their consistency with the empirically validated $\lambda<0$ mechanical framework, raising none of the problems Lorentz and Poincaré had found themselves battling against, until Einstein arrived and cut the Gordian knot with the sharp blade of his second postulate.

Let's briefly consider the kind of dynamics that complies with a κ-transformation. Assuming that the principle of inertia remains valid, the laws of mechanics must be reconsidered to take into account the connection such a transformation establishes between space and time variables. The simple Newtonian mass×velocity definition of momentum turns out to be inconsistent with the non-additive law for composing velocities. Defining momentum as $\mathbf{p} = \gamma m\mathbf{v}$ is a convenient choice that yields the classical Newtonian form in the $\frac{v}{\kappa} \to 0$ limit. The energy variation dE for a system that undergoes an infinitesimal displacement d$\mathbf{r}$ is by definition such that $\mathrm{d}E = \frac{\mathrm{d}\mathbf{p}}{\mathrm{d}t}.\mathrm{d}\mathbf{r}$. Writing the equation $\mathrm{d}\left(\frac{E}{\kappa}\right)\mathrm{d}(\kappa t) - \mathrm{d}\mathbf{p}.\mathrm{d}\mathbf{r} = 0$ suggests a parallel between $\frac{E}{\kappa}$ and $\mathbf{p}$ on the one hand, and κt and $\mathbf{r}$ on the other, where the latter are connected by a κ-transformation. Most significantly, the terms in each product are 'Noether pairs' that each comprise a variable (respectively t, $\mathbf{r}$) and a physical quantity (respectively E, $\mathbf{p}$) such that the invariance of dynamics under a shift in the value of the variable is signalled by conservation of the associated quantity. The left-hand side of the equation has the same form as the kinematical invariant $\mathrm{d}(\kappa t)^2 - \mathrm{d}\mathbf{r}^2$, which is implied by the κ-transformation law. This 'scalar product' form and the aforementioned duality suggest that energy and momentum transform according to the same κ-transformation as time and space

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synchronised (that his own clocks were clepsydrae only makes it somewhat impractical for deciding between the $\lambda=0$ and the $\lambda<0$ variants of relativity theory…). See Feigenbaum, *op.cit.* for a brief discussion of Galileo's procedure.

[32] Shouldn't we be just as surprised to see, on the standard account, a 'strange' aspect of light determine the basic transformation rule of a relativity theory? Why κ happens to coincide with the speed of light is just as much an enigma today as it was in 1905 (and it is *not* resolved by invoking the zero mass of the photon).

coordinates. If so, then given the above definition of **p**, the expression of energy in terms of mass, velocity and the universal constant κ is $E = \gamma m\kappa^2$. Again, identification of the value of κ with the speed c of light is an empirical matter. There is no clear reason – to say the least – why the two should actually coincide, nor is it expected a priori that κ should be 'actualised' in any kind of physical phenomenon[33].

The resulting 'topochronometric' framework ensures that any well-defined physical quantities will systematically and correctly transform upon switching from one inertial frame to another. The framework operates, in accordance with the prerequisites of a relativistic programme, without any reference to specific interactions or entities. That theoretical expressions should not, other than trivially, depend on mere changes of (kinematical) 'perspective' is a necessary condition for reliable communication, intersubjective agreement and the legitimate identification of certain relations as expressions of genuine physical laws. Those cognitive-operational requisites are part and parcel of ongoing efforts to achieve a robust, intersubjectively valid account of phenomena. Among the assumptions that lead to the derivation of the λ-transformation, isotropy and homogeneity amount to denying any world-based particularity that would lessen the general validity of the outcome. They are formulated at a level of abstraction where they acquire a virtually 'transcendental' status[34]. In contrast to Einstein's 'strange' claim about light, those assumptions have a sensible – indeed, rather anticlimactic – character, but the pay-off is considerable and immediate: the form of possible transformations is so contrained that the additional invocation of transitivity and reversibility (via group-theoretical composition) is all it takes to complete the derivation.

That the Galileo and the Lorentz transformation are actually derived from the same set of assumptions makes it necessary to reevaluate the relation between the Newtonian and STR frameworks. Their actual order of succession is easily accounted for: the Galilean option is, of course, that which dovetails with our ordinary experience, hence with corresponding expectations. Experimentation with advanced means is required for revealing any discrepancy with the assumed λ=0 case and for giving empirical backing to the only acceptable (λ<0) alternative. Indeed, it took 'anomalies' of electromagnetism (from a Galilean viewpoint) and the 'lucky coincidence' of κ with the speed of light for the Galilean option (never deemed an 'option' until then) to be challenged and finally overthrown.

A realist might still insist that the world *is*, as a matter of fact, $\{\lambda<0, \kappa=c\}$-relativistic. One of the two possibilities must somehow have been objectively 'selected', and our conceptual expectations can have nothing to do with the matter. Doesn't the identity of κ

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[33] Discovering any (minute) discrepancy between κ and c would undermine Einstein's two-postulate formulation of STR. The generic λ<0 relativistic framework, however, would retain its validity. The question would remain as to why the values of κ and c, whilst not coinciding, should be so close to each other.

[34] Borrowing the word should not, of course, be mistaken for endorsing any post-Kantian view of cognition.

and c point to such natural underpinnings? Numerical models hint at an intriguing alternative: the world may not so much be 'relativistic', i.e. non-Galilean, than we are bound, qua observers, to experience 'it' as such. Starting from a random distribution within a finite radius, a random walk model whose dynamics is not rotationally invariant at the micro-level leads to the emergence, in the long run, of a distribution that is invariant under the continuous group of rotations. Assuming equal probabilities for 'opposite' motions in the lattice, a Galilean transformation slows down the evolution[35] of the binomial distribution by a factor γ^{-2}. It takes a 'Lorentz' transformation, followed by a further scaling by a factor γ^{-1}, to counteract this effect and to restore the dynamics. A more refined lattice-gas model of diffusion[36] yields similar though indirect results. Here, dynamics is deterministic and reversible at the micro-level, but it gives rise to just the same macro-level phenomenology as the random walk. The system is a one-dimensional cellular automaton with a given lattice spacing. As the spacing goes to zero (continuum limit), this model exhibits exact Lorentz covariance in the following sense: starting from an initially uniform distribution with fixed linear density, the probability distribution converges in the continuum limit toward the solution of the telegrapher's equation[37] – an equation that was derived by Heaviside in the 1880s as an essential part of his transmission line model. Its form can be traced back to Maxwell's equations, from which it inherits the 'built-in' property of Lorentz invariance. It is also closely related to that of the Klein-Gordon equation, which provides a non-Galilean ($\kappa = c$) version of Schrödinger's equation that is suited for the treatment of a spinless system[38]. This computational model, together with other simulations[39], suggests that meeting the requirements of STR might be an emergent phenomenon, to which the actual nature of the underlying dynamics is to a large extent if not entirely irrelevant. It is intriguing, though certainly not quite to the realist's taste, to contemplate the possibility that Lorenz covariance might be the only form of relativity that is compatible with our situation

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[35] In this model, 'proper time' is measured by the standard deviation. T. Toffoli, 'How Cheap Can Mechanics' First Principles Be?', in W.H. Zurek, (ed.): *Complexity, Entropy and the Physics of Information: Proceedings of the Santa Fe Institute Studies in the Sciences of Complexity*, Vol.8, Redwood City CA: Addison-Wesley 1990, pp.301-318, 4.1.

[36] T. Toffoli, *op.cit.*, 4.2.

[37] The same distribution also converges, in the infinite time limit, to the solution of the diffusion equation – another partial differential equation (so does the binomial distribution).

[38] Another equation with a similar form is the so-called *hyperbolic heat equation*, which was introduced to overcome the inconsistency of Fourier's equation with the requirements of STR. This is achieved by 'upgrading' the parabolic form of Fourier's equation and making it hyperbolic, so that it is compatible with the non-positive metric of STR (the Lorenz transformation can be regarded as a hyperbolic rather than circular rotation, because of a one-one mapping between a planar rotation through a given angle and a Lorentz transformation with a given velocity).

[39] T. Toffoli, 'Four Topics in Lattice Gases: Ergodicity, Relativity, Information Flow and Rule Compression for Parallel Lattice-Gas Machines, in R. Monaco (ed.), *Discrete Kinetic Theory, Lattice Gas Dynamics and Foundations of Hydrodynamics*, Singapore: World Scientific 1989, pp.343-354.

as macroscopic agents and observers, alhough our low-velocity ordinary regime tends to impose, deceptively but usefully enough, a Galilean perspective.

(ii) Another kind of probability calculus

However much is being said about the seemingly incurable, alleged weirdness of quantum theory, there are compelling grounds for regarding its mathematical backbone (we shall refer to it as the Statistical Algorithm of Quantum Mechanics[40] [SAQM]) as nothing but a linear variant of ordinary probability calculus. Recent years have witnessed some intriguing and certainly significant attempts at deriving basic features of the SAQM, from the combination of amplitudes to the Born rule, from simple sets of well-motivated, *physically neutral* assumptions. Some of the more striking derivations will be outlined below, with a view to weighing their outcome against the claims of the structuralist.

In a fashion that is reminiscent of the approach to relativity discussed in section 4(i), Lucien Hardy has recently derived *both* the SAQM and a vector formulation of classical probability calculus from four basic and straightforward axioms[41]. Hardy's contribution starts on with a simple observation: the probabilities of a complete set of K mutually exclusive measurement outcomes can, with no loss of generality, be written down as components of a K-dimensional vector. 'Measurement' here is to be understood in the most general sense of collecting data through some appropriate device or procedure. Now, any single trial or 'single shot' measurement can at most distinguish between N outcomes. Hardy's first axiom asserts the existence of a functional relation between K and N, which moreover should be the simplest possible (there is no reason for assuming that K and N should be *a priori* equal). Constraints will then typically reduce the number of possible outcomes, and it is sensible to assume that the dimension K of the probability vector space should be reduced accordingly (this is Hardy's second axiom). In the case of simultaneous dice throws, of jointly produced pairs of particles etc., how do the K and N for pairs relate to those that correspond to measurements performed on their members separately? If measurements performed on each subsystem, and on that subsystem alone, suffice to determine the corresponding probability vector, then it is sensible to assume that such measurements are, together, sufficient to completely determine the probability vector that

[40] M.L.G. Redhead, *Incompleteness, Nonlocality and Realism*, Oxford: Clarendon Press 1987, p.5.

[41] L. Hardy, 'Quantum Theory from Five Reasonable Axioms', Arxiv.org e-print quant-ph/0101012 (2001); L. Hardy, 'Why is Nature described by Quantum Theory?', in J. D. Barrow, P.C.W. Davies and C.L. Harper Jr (eds) : *Science and Ultimate Reality: Quantum theory, Cosmology and Complexity*, Cambridge: Cambridge University Press 2004, pp.45-71. One of Hardy's five axioms can actually be dispensed with, expressing as it does the author's commitment to a frequentist (rather than e.g., Bayesian) interpretation of probability. One's position in this matter is actually irrelevant to the derivation of the SAQM.

pertains to the combined system ('local accessibility' thesis[42]). Letting K_1 and K_2 be the dimensions of the probability vector spaces relative to each pair member, and N_1 and N_2 be the related N (through Axiom 1), the assumption amounts to letting $K = K_1K_2$ and $N=N_1N_2$ (Axiom 3). The underlying intuition is that a scheme that implements local accessibility will make optimal use of the information supplied by measurements performed separately on the two subsystems.

Together, the three axioms entail that $K = N^r$, where r is a positive integer. The simplicity requirement of Axiom 1 further restricts the possible values of r to 1 and 2. The $r=1$ ($K = N$) case is nothing but a vector space formulation of classical probability theory, whereas $r=2$ yields all the essential features of the SAQM. In both cases the probability vectors evolve between observations in a fashion that does preserve, as required, the basic (e.g. adding-up-to-1) properties of probability. 'Reduction' of the probability vector in the $r=2$ case simply signals the updating of probability upon acquisition of new information. Besides, Hardy's derivation settles a vexed issue: why the SAQM should require Hilbert spaces to be defined over the field of *complex* numbers. As it turns out, real Hilbert spaces cannot accommodate local accessibility: the specification of probability vectors in the bipartite case would then require more ($K>K_1K_2$) than can provide data gathered locally on the two subsystems.

The very fact that the SAQM is actually derived from the same set of assumptions as (a vector implementation of) classical probability calculus, where the latter is clearly not a physical theory, throws doubt on a widespread opinion: that the basic features of the SAQM somehow 'reflect' the 'quantal' nature, or the 'quantal structure', of the physical world. Given the composition of the axioms and the generality of their double-headed outcome, we might rather expect a $r=2$ scheme to have potentially wider applicability than one might have surmised, given our knowledge of one instance only of such a scheme, namely quantum mechanics. And one can indeed find some successful, or at least promising efforts to develop, in domains far removed from physics, a useful probabilistic framework that is isomorphic to the SAQM[43]. When it comes to drawing the line between the two options, Hardy reduces the matter to acceptance or rejection of a continuity requirement: in a $r=2$ framework, any probability vector can be continuously and reversibly transformed into any number of other probability vectors – this is indeed what 'superpositions' are all about. The 'classical' ($r=1$) alternative excludes the possibility of

[42] W.K. Wootters, 'Local Accessibility of Quantum States', in W.H. Zurek (ed.): *Complexity, Entropy and the Physics of Information: Proceedings of the Santa Fe Institute Studies in the Sciences of Complexity*, Vol.8, Redwood City CA: Addison-Wesley 1990, pp.39-46.

[43] See in particular D. Aerts and L. Gabora, 'A Theory of Concepts and their Combination. I: The Structure of the Sets of Contexts and Properties & II: A Hilbert Space Representation', *Kybernetes* **34**, pp.167-191 & pp.192-221 (2005) [ArXiv.org e-print quant-ph/0402207 & quant-ph/0402205]. The authors develop a scheme, the logico-algebraic (lattice) structure of which matches that of the SAQM, and in which probabilities are computed according to the usual, basic 'quantum' rules. The purpose of that scheme is to yield statistics of concepts on the basis of certain criteria, such as their typicality (so that e.g. 'snake', or even 'goldfish', is a less typical, hence lower probability instance of 'pet' than 'cat') .

any such transformation (which makes it, in a sense, less 'natural' than the other option: one hardly feels the need for a *vector* formulation of ordinary probability calculus). Bearing in mind that probability vectors need not be regarded as representatives of 'states' (a word that is dangerously loaded with ontological overtones), there is little reason for claiming that the continuity-based $r=1$ vs. $r=2$ distinction 'reflects' anything essential about an objective physical world. No more, indeed, than we can find any reason to do so when it comes to distinguishing a priori between the two relativistic $\lambda=0$ and $\lambda<0$ options.

Besides Hardy's, other contributions support a view of the SAQM as a specific type of probabilistic framework. Let us start with the idea of labelling with 'amplitudes', i.e. real or possibly complex numbers, a connection we establish, for purposes of prediction, between an 'initial preparation' (I) and a 'target event' (T). Given an intermediate event S, such that it *can* – but it may not always – be ascertained that the I$\rightarrow$T transition does proceed via S, it is reasonable to expect the I$\rightarrow$T amplitude to be a function f of the 'partial' amplitudes one assigns to I$\rightarrow$S and to S$\rightarrow$T. Likewise, denoting by {S,S'} a complete set of mutually exclusive 'intermediate' events, the 'total' I$\rightarrow$T amplitude will be a function g of the amplitudes associated with the I$\rightarrow$S$\rightarrow$T and I$\rightarrow$S'$\rightarrow$T sequences. Consistent assignment then requires both functions to be associative and f to be distributive over g. Assuming those functions are analytical, then Feynman's rules for the combination of amplitudes follow: the total amplitude for a permissible sequence is the product of the amplitudes of its terms, and amplitudes of mutually exclusive terms or sequences add up to yield a total amplitude[44]. One further step towards 'recovery' of the SAQM consists in letting amplitudes be the coordinates of suitable vectors[45]. Requiring that probability, as a function of amplitude, retain its form under vector basis changes implies a power form relationship of probability to amplitude. This form will be consistent with the metric structure of the representative space, associated with the definition of a scalar product and norm, if and only if probability relates to amplitude as its squared modulus[46]. In contrast to Hardy's, it is a shortcoming of this approach that the nature of the field over which the metric vector space (Hilbert space) must be defined remains undetermined.

Besides, the question arises of why one should bother at all to introduce amplitudes as an auxiliary device for prediction, i.e. for the calculation of probabilities. Hardy's derivation, among others, suggests that resorting to amplitudes affords a means of enhancing statistical

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[44] Y. Tikochinsky, 'On the generalized multiplication and addition of complex numbers', *Journal of Mathematical Physics* **29** (2), 398-399 (1988); Y. Tikochinsky, 'Feynman Rules for Probability Amplitudes', *International Journal of Theoretical Physics* **27** (5), 543-549 (1988). A. Caticha, 'Consistency, amplitudes, and probabilities in quantum theory', *Physical Review* A**57**, 1572 (1998), ArXiv.org e-print quant-ph/9804012.

[45] These should not be confused with Hardy's vectors, whose components are probabilities.

[46] One early argument to that effect is found in P. Destouches-Février, 'Signification profonde du principe de décomposition spectrale', *Comptes Rendus de l'Académie des Sciences* **222**, 866-868 (1946); see also P. Destouches-Février, *L'interprétation physique de la mécanique ondulatoire et des théories quantiques*, Paris : Gauthier-Villars 1956.

distinguishability. Suppose that one decides to test three variables on a population. The probability triple for each population can be represented as a point on a triangle or simplex. Letting each probability be the square of a (real) amplitude, the amplitude triples then lie on a curvilinear triangle, which is ‘stretched’ relative to the simplex. If two populations **A** and **B** are such that probabilities assigned to some of their features appear to be closer than those for two other populations **C** and **D**, as shown by the distance between the corresponding points on the simplex, then **A** and **B** might seem to be harder to distinguish from each other than **C** from **D** on the basis of the considered variables. Statistical analysis will, however, sometimes belie that impression. Whenever this occurs, switching to amplitudes shows that the apparent difference is a mere artefact of the linear representation (on the curved triangle, the distance between **A** and **B** may in fact be the same[47] as that between **C** and **D**). Although much clarification is needed here, this suggests that optimising the distinguishability of probability distributions is a key trait of a predictive scheme like the SAQM, whether its formulation explicitly makes use of amplitudes (whose modulus, equivalent to Hilbert space angle, provides a measure of statistical distance[48]) or it takes the general form of a $r=2$ framework.

Granted that its ‘nonclassical’ features are those of a specific kind of probabilistic algorithm, quantum mechanics is nonetheless a physical theory. What makes it such owes everything to the linear representation of groups. Group-theoretical considerations shape and constrain physical quantities, the distribution of their possible (in-principle measurable) values and their mutual relationships. Given the specific structure of the SAQM, they also determine the relative phases of amplitudes and the fact that those amplitudes *are* more generally complex[49] (that they *must* be in general follows, as we have seen, from a local accessibility assumption). Fundamental types of invariance in pre-quantum mechanics are connected to space-time symmetries associated with translations, rotations and inertial transformations (STR substitutes the Poincaré group for the Galileo group). In quantum mechanics, such symmetries apply in conjunction with the linear and projective structure of the SAQM to determine the form of the operator representatives of physical quantities, hence the constitution of their spectra. If the modulus of the inner product of any two predictive vectors, and therefore probability, is to remain unaffected by symmetry transformations of Hilbert space, then Wigner’s theorem implies that those transformations must be implemented by a unitary or an anti-unitary operator. If such transformations form a group, then the corresponding operators must satisfy the

[47] For an example, see J.A. Wheeler, ‘World as System Self-Synthesized by Quantum Networking, *IBM Journal of Research and Development* **32**, 4-15 (1988).

[48] W.K. Wootters, ‘Statistical distance and Hilbert space’, *Physical Review* D **23** No2, 357-362 (1981).

[49] Complex numbers are required e.g. for the application of the SAQM to the treatment of spin-½ systems. The need for spin-related amplitudes to be generally complex is a direct consequence of the linear group representation of three-dimensional rotations. See R.P. Feynman, R. Leighton and M. Sands: *The Feynman Lectures on Physics*, Vol. 3: Quantum Mechanics, Reading MA: Addison-Wesley 1965, Chapter 6; R.I.G. Hughes, *The Structure and Interpretation of Quantum Mechanics*, Cambridge MA: Harvard University Press 1989, pp.127-135.

multiplicative group law, and there must exist a unitary projective representation of the group in the Hilbert space 'of' the system. Which types of systems are possible comes down to the types of linear group representations that can be accommodated. In the case of space-time symmetries, the relevant groups are continuous Lie groups, whose representations can only be unitary. The Stone-Naimark theorem then guarantees the existence, for any one-dimensional subgroup of the symmetry group, of a self-adjoint operator that is the infinitesimal generator of the group, whereby a representation of the Lie algebra of the symmetry group obtains[50].

Insofar as the self-adjoint operators are expressions of invariance under space-time transformations, they fulfil the most elementary requirements for being regarded as representatives of basic physical quantities. The association, via Noether's theorem, of continuous space-time symmetries with conservation laws – of energy with time shifts, of angular momentum with rotations etc. – provides the best available justification for the usual practice of referring to those operators using the names of their classical analogues: Hamiltonian, angular momentum etc. Such terminological usage may encourage unwarranted expectations and lead to puzzlement when those are not fulfilled. Nevertheless, its legitimacy is not ultimately based upon vague analogies or putative 'correspondence' with classical physics. Losing a sense of 'intuitiveness' or *Anschaulichkeit* might actually be a fairly low price to pay for the group-theoretical foundation of basic quantities like linear and angular momentum to be put on a firm basis.

If something substantial ends up being carried over, albeit not merely 'retained', from classical to quantum mechanics, enabling 'correspondence' and fuelling hopes among structuralists, this is because those two frameworks, despite all their dissimilarities, must hinge on similar prerequisites of theorisation (e.g. regarding spatio-temporal or other types of invariance). In the quantum mechanical setting, the familiar descriptive, *anschaulich* style of classical mechanics gives way to the blind efficiency of a probabilistic algorithm (SAQM), whose symbolic machinery is brought to bear on group-theoretically regulated physical quantities. The classical and 'quantal' ways of handling those essential prerequisites and of developing their consequences are too dissimilar for classical relations to actually 'survive' the change. Thus, Newton's Second Law and the expression of force as the gradient of potential are 'recovered' only as the outcome of an averaging procedure (Ehrenfest's theorem). If this and other correspondence rules are convenient shortcuts, a thoroughly group-theoretical approach is required for quantities without a classical analogue, like spin, to be accounted for (the latter emerges as another variety of angular momentum, with its fundamental association to rotation). Yet another weakness of

[50] Since the operators and their mutual relationships are in direct correspondence with the Lie algebra structure, physicists seldom bother to distinguish between the Lie algebra elements and their operator representatives.

correspondence claims is that quantum mechanical treatments give rise to a number of 'classical limits' whose mutual consistency is far from guaranteed[51].

Hardy's axioms are just as operational as are the assumptions from which the joint derivations of the Lorentz and Galileo transformations follow. Nothing in them clearly or plausibly points to any 'pressure' from a pre-existing and pre-structured external world, which would somehow result in a $r=2$ scheme being enforced as 'nature's choice'. A key to the efficiency of the SAQM may well be its capacity to optimise the distinguishability of probability distributions, as encapsulated e.g. in a Hardy vector. Can this capacity make sense as an inherent trait of external, 'observer-free' reality? As for those considerations of symmetry and invariance that determine and guarantee the physicality of both the classical and the quantum mechanical frameworks, the possibility of imputing them all to the constitution of the physical world remains open, but neither more nor less than they had been before the emergence of the quantum. However, the justification of conservation laws as reflections of invariance under space or time shifts, rotations and so forth might just as well, if not better, be regarded as expressing essential, part cognitive, part operational requirements for intelligibility and predictive effectiveness.

5. On the dubious value of the structuralist's hunches

The observed 'resilience' of certain equations and formulae strongly suggests that theoretical accounts of physical phenomena are to a large extent insensitive to metaphysical tenets and ontological preferences. Far from being helpful, clinging to certain kinds of entities or processes is more likely than not to become obstructive[52]. However, certain 'metaphysical' preconceptions also act as motivators for coming up with assumptions whose selection will determine the success of a particular account of phenomena. Assuming, for instance, a certain mode of space or time variation, with attendant prerequisites of uniformity etc., implies the selection and use of appropriate types of mathematical objects, e.g. a divergence or a Laplace operator, such that the 'behaviour' of a system will be expressed as relations (e.g., differential equations) that involve those objects. Following 'natural' human inclinations, credit for any significant success will be given to the postulated ontology… until further developments make that particular ontology untenable, despite the predictive success of the models it fostered.

Now, whatever ideas happen to 'drive' the physicist, effective theorisation is the product of painstaking, focused effort. It requires an unequivocally precise formalisation of ideal 'systems', subject to clearly delineated and articulated conditions and constraints, all of

[51] See e.g. R.L. Liboff, 'The correspondence principle revisited', *Physics Today*, February 1984, 50-55.

[52] This is one basic motive for Duhem's rejection of 'explanation' as the aim of a physical theory (Duhem, *op.cit.*).

which must comply with basic assumptions of accepted frame theories. Such a programme must also face the necessity of arriving at equations that can be solved, or at least exploited in certain useful ways. Because of all those needs and concessions, widespread similarities in theoretical, mathematised treatment and its outcome (e.g. partial differential equations of the same form) are inevitable; and this is so regardless of glaring dissimilarities between the actual objects of those treatments[53]. Resemblances in mathematical formulation and processing can always be traced back to conceptual and methodological decisions that determine whether any given model is to be successful.

Structuralists wish to capitalise (TTR) on the actual 'recovery' of formulae that pertain to former frameworks, from expressions derived using the resources of their radically different successors. The joint derivation of both the Galileo and the Lorentz-Einstein transformation, *from the very same set of assumptions*, should make it clear, however, that this is not merely a case of the later framework having (somehow) *retained* something significant from the former. Neither can it suffice to point out that such retention, or whatever commonality between the two frameworks the word *retention* is intended to highlight, would operate at a 'structural level'. Upon closer scrutiny, the two change-of-frame transformations turn out to be alternative outcomes of expecting a transformation rule to satisfy a minimal number of sensible and operationally motivated requirements. There are no serious grounds for seeing in those assumptions, as the structuralist invites us to infer (thesis (iii)), any putative 'reflection' of the structure of the world, whatever clear meaning this expression can be given.

Likewise, there are some compelling reasons for believing that the basic features of the SAQM are responses to requiring a probabilistic, linear scheme for prediction to satisfy elementary demands, which may come down to rather straighforward matters of distinguishability and of local accessibility. The vector character of this type of framework may well be optimal given the linear representation of groups, from which quantum theories derive their physical content. There again, there is no clear sense in which quantum-mechanical structure should be thought of as mirroring a correspondingly quantal structure of the natural world, and there certainly is no conceptual, epistemological advantage in following the structuralist down a road that looks like nothing more than a metaphysical dead end.

One must beware of a seldom resisted temptation, which is to reify invariant (or suitably covariant) quantities, and to believe that they offer glimpses of a stable and objective, observation or abstraction-independent reality. The invariants the physicist is concerned with are such only given specified, or implicitly assumed, experimental or perceptual conditions – what may be referred to as a *context*. Despite a tendency to focus on the

[53] Beyond physics, crowd behaviour or road traffic can be modelled using adaptations of the mathematical tools that were developed towards the end of the 19^{th} century for the statistical treatment of gases. Such modelling can be remarkably (surprisingly?) informative. For a non-technical presentation, see P. Ball, *Critical Mass: How One Thing Leads to Another*, London: Arrow Books, 2005.

entities themselves as the locus or source of invariance, the latter is a structural property of a set of *operations* (usually formalised in terms of groups) rather than anything externally enforced 'by the world'. It is thus illegitimate to invoke any kind of invariance or systematic change (captured by some definite rules) as evidence of theories latching on to the world .

However well-reasoned our scepticism may be, it is most unlikely that the structuralist will be willing to come to terms with the possibility that the conceptual or structural content of physical theories may *not* 'mirror' relational aspects of an external world but, *nonetheless*, be effective and intellectually worthwhile. For it is his/her '*default* assumption', as John Worrall puts it, that given appearances of TTR between predictively successful theories, those theories 'have latched on *in some way* to the 'deep structure' of the universe[54].' Promoting realist hunches may not, however, be the best way of achieving a deep understanding of physical theories, and of helping elucidate how their mathematisation can make them so *reasonably* effective.

[54] J. Worrall (2007), *op. cit.* p. 147, italics added.